DOCUMENT RESUME

ED 056 697

LI 003 205

AUTHOR	Crawford, Susan, Ed.
TITLE	Informal Communication Among Scientists: Proceedings of a Conference on Current Research.
INSTITUTION	American Medical Association, Chicago, Ill.
PUB DATE	22 Feb 71
NOTE	50p.; (7 References)
EDRS PRICE	MF-$0.65 HC-$3.29
DESCRIPTORS	*Communication (Thought Transfer); Conferences; *Informal Organization; *Information Seeking; Information Sources; *Research; *Scientists; Social Sciences
IDENTIFIERS	*Invisible Colleges

ABSTRACT

On February 22, 1971, a meeting of investigators studying informal communication among scientists was held at the American Medical Association. The participants were limited to ten members in order to preserve a seminar-type format. The meeting was led by Derek Price, and Fred Strodtbeck, an authority on small groups, was invited as resource scientist. Besides a list of the participants, the "Proceedings" of the meeting include major presentations by Drs. Price and Strodtbeck, discussion of these papers and resumes of work submitted by the attendants. (Author/NH)

ED 056697

U.S. DEPARTMENT OF HEALTH,
EDUCATION & WELFARE
OFFICE OF EDUCATION
THIS DOCUMENT HAS BEEN REPRODUCED EXACTLY AS RECEIVED FROM THE PERSON OR ORGANIZATION ORIGINATING IT. POINTS OF VIEW OR OPINIONS STATED DO NOT NECESSARILY REPRESENT OFFICIAL OFFICE OF EDUCATION POSITION OR POLICY

INFORMAL COMMUNICATION AMONG SCIENTISTS: PROCEEDINGS OF A CONFERENCE ON CURRENT RESEARCH

AMERICAN MEDICAL ASSOCIATION
FEBRUARY 22, 1971

LI 003 205

Introduction

In November 1966, Derek Price called a meeting of the "invisible college of scientists studying invisible colleges." This small group which consisted of six doctoral students identified a total of 20 scientists who were working in this area.

Since this time these investigators have completed their projects and moved to various academic institutions. We felt that it was appropriate to bring this group together again to report their results, to discuss their thinking in this area, and to find out about their current work.

On February 22, 1971, a meeting of investigators studying informal communication among scientists was held at the American Medical Association. The participants were limited to ten members in order to preserve a seminar-type format. The meeting was led by Derek Price, and Fred Strodtbeck, an authority on small groups, was invited as resource scientist.

The Proceedings of the meeting include major presentations by Drs. Price and Strodtbeck, discussion of these papers and résumés of work submitted by the attendants.

Susan Crawford, Editor

Participants

1. Diana Crane, Associate Professor,
   Department of Behavioral Sciences,
   Johns Hopkins University

2. Susan Crawford, Director,
   Archive-Library Department,
   American Medical Association

3. Bernard Gustin, University of Chicago

4. David Lingwood, Center for Research and
   Utilization of Scientific Knowledge
   University of Michigan

5. Nicholas Mullins, Assistant Professor
   Department of Sociology
   University of Indiana

6. Derek de Solla Price, Avalon Professor of the
   History of Science
   Yale University

7. Fred Strodtbeck, Professor of
   Social Psychology
   University of Chicago

8. Dave Vachon, Assistant Professor
   School for Public Communication
   Boston University

9. Gerald Zaltman, Associate Professor
   Department of Marketing
   Northwestern University

Table of Contents

Introduction..... 1

Invisible College Research: State of the Art

Derek Price..... 3

Perspectives on Communication Research from the Viewpoint of A Social Scientist

Fred Strodtbeck.....14

Summaries of Current or Completed Research.....27

Invisible College Research: State of the Art

Derek Price

"In the "Cloisters" of the Metropolitan Museum in New York there hangs a magnificent tapestry which tells the tale of the Unicorn. At the end we see the miraculous animal captured, gracefully resigned to his fate, standing in an enclosure surrounded by a neat little fence. This picture may serve as a simile for what we have attempted here. We have artfully erected from small bits of evidence the fence inside which we hope to have enclosed what may appear as a possible, living creature. Reality, however, may be vastly different from the product of our imagination; perhaps it is vain to hope for anything more than a picture which is pleasing to the constructive mind when we try to restore the past." -- The Exact Sciences in Antiquity - Neugebauer

Invisible college research, seems to me, rather like hunting the unicorn. Somehow, we got word of the existence of such a beast, and we set out to hunt it in various ways appropriate to hunting something which would be like a sociological clique or peer group. Each of us latched on to one or two different specimens, and then we had to decide what the object was that we had indeed hunted down. In retrospect, we can see that it would not have been useful at all to decide on its properties before hunting it.

To continue this rather picturesque analogy - having caught the unicorn, we looked at it and found that it was a perfectly ordinary animal, a normal sociological clique, a group having properties similar to other groups generated in the same manner. What keeps eluding us is just what it is that makes this group different from other social groups. We now know much more about the invisible colleges than we know how to describe them.

The conceptual models we need for proper description are lacking. Our mathematical analyses, in spite of all the advances that have been made and the individual idiosyncratic ways that each of us has tried, are grossly inefficient. Our next step is obviously to try the one hundred and one things that a bright statistician might do. The summary tables and charts derived from a great mass of generated data

have somehow lost results which we thought to exist. What has so far come out of these studies?

There is clearly some sort of core within a larger group. There is also some sort of elitism. The groups we are looking at are social elites - based upon ability and power; they have been called in-groups, gatekeepers, peer groups, and are indeed also peerage-conferring groups. They may be stratified by age, experience, power and other variables. What appears to be special are some social and intellectual properties of science which operate to distinguish scientists from other social groups.

1. These groups are in course of more rapid, exponential growth than is usual for the more "scholarly" groups or social groups in the community. There is a 2-4% annual increase of members in the latter, in contrast with something like a 7-10% increase in our groups. Such rapid growth gives us a different perspective. There are also more young people - in age or length of experience in the field - than one would find in a cross section of the general population.
2. Scientists are immersed in a universal system which is much more competitive than all other human activities such as, for example, the athletic or business sectors. Because of its objectivity, impersonality, cumulative nature and relative certainty, very quick and highly credible judgements are possible in science. There is some difference, however, between the "hard sciences" and the "soft sciences." In general,

the harder the science, the easier it is to size up very rapidly and surely the contribution of a scientist. Science has also a much tighter structure than non-science. One of our most considerable remaining problems seems to be the definition of what is meant here by "structure" and the "tightness of structure."

3. Science, because of its structure, "grows from the skin." You have what Kuhn calls "paradigms" of normal science. There is also a packing-down process in which the research skin is compressed and compacted so that it can rapidly be rehearsed by novitiates. Somehow, this is different from what goes on in non-scientific scholarship, the soft sciences, and to some extent the medical and engineering professions.
4. There may be some differences between groups in the mechanics of the "core". In our work, we have shown that a subset which is in number the square root of the group population produces about one half of the work. This has not been made use of or understood by some sociologists, because people keep talking about a core as if it were a fixed proportion, say 5 to 10% of the group. A group of 100 has a core of ten people (10%), but a group of 10,000 has a core of only 100 people (1% of group). Probably what happens is that the size of the group determines the level at which the core cuts off, and a large group has a smaller proportion than a small group. The Coles kept finding that the size of the core fluctuated and they kept trying to

find something which they thought should be constant. I am not at all sure whether this is a fundamental property of science or common to the sociometry of large and small groups. It is certainly affected by technique of analysis, and there are some difficult methodological problems to be solved.

5. The one substantial point which I would like to make is that science contains a much larger flow-through of people than any other social group I have encountered. Figure 1 illustrates the attrition rate.

We took five years of the most comprehensive indices of publishing scientists known to us - the International Directory of Research and Development Scientists which is based upon all the names which occur in Current Contents and the Citation Index. We combined the names with the Citation Index to include not only all authors and their collaborators, but also those who were cited. An effective spread of eleven years was derived - five years before any particular date and five years after. We then examined what happened to the population of scientists, old and new, and investigated the probability of names turning up again and again.

At any given time, there are many people involved in science - in any particular science or in all the sciences. Our population consisted

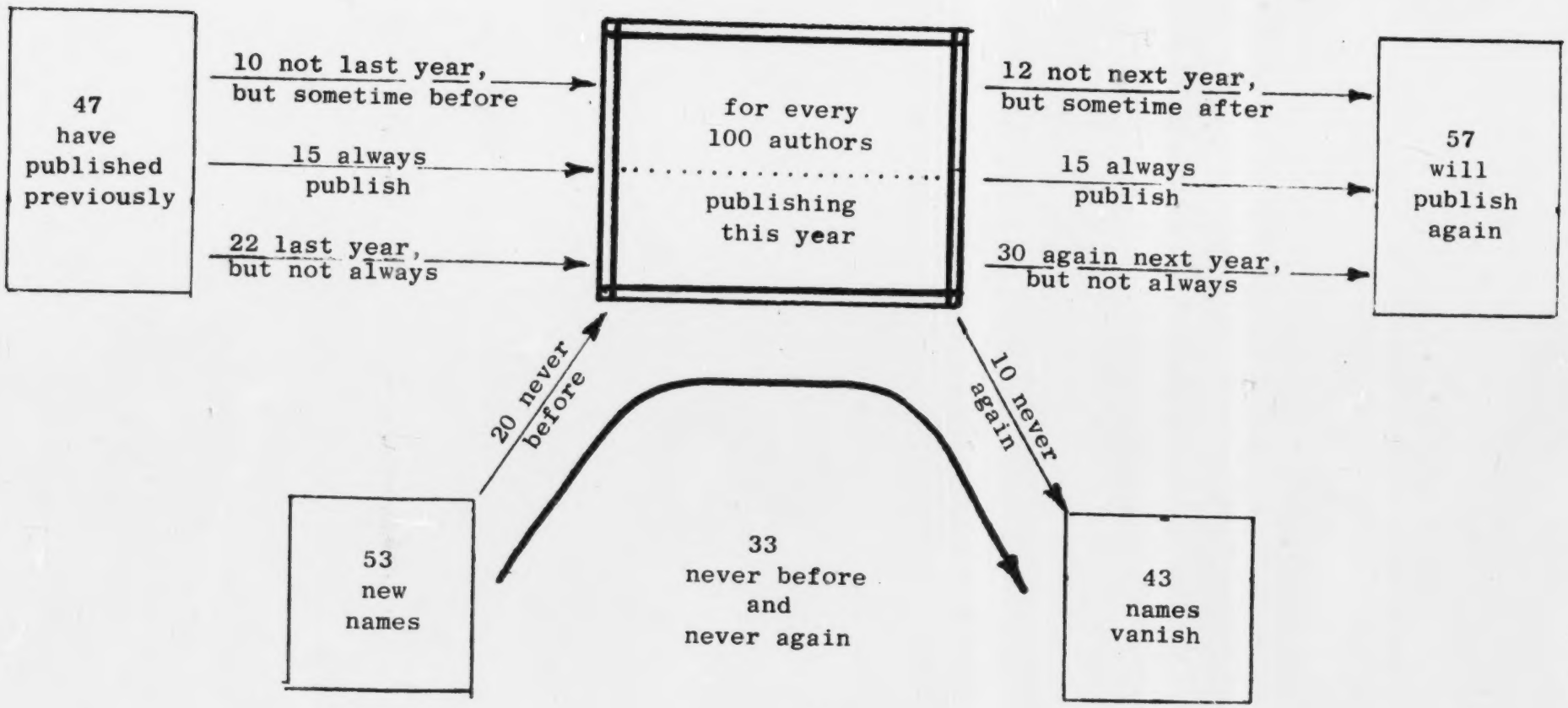

Figure 1

Data from 1966 ISI
with surrounding data
for 1964,'65,'67,'68

Derek J. de Solla Price

February 1971

of millions of names, so we selected all authors within the alphabetical range of PAI to PAM, and this came out to a sample of 500 main authors and about 1,000 subsidiary ones.

If we focus upon any 100 names on a given list and observe their movement in and out of the group, we will find that 47 of the 100 names have occurred previously and 53 have never occurred before. Going forward from the 100 names, we found that 57, over a half, will occur again. The ten extra names constitute the growth rate of 10%, and 43 of the names vanish.

We next looked at the two compartments of 53 names that have never appeared before and the 43 that never appear again. There are 33 names (one third) which have both never appeared before and will never appear again. This is not to say that these people are wasted; they have obviously gone on to something different. If we focused upon patents rather than scholarly publications, we will again find that a third of the people appearing in patents in a given year have never been in patents before and never will be in patents again. Apart from the 33 names, there are 20 new names which will appear again. At this point, ten of the old names that had been appearing die. What we have is the pathological condition of a birth rate of 20% and death rate of 10%. Consider as a model, a very primitive village with every woman giving birth at the maximum rate. Public health is abysmally poor and nearly everybody dies. At any time, the population consists of children who are just passing through this existence.

Science is remarkably like this - most of the people are just passing through. The birth rate is twice the death rate. The same conditions extend when you consider the 47 persons who have published before and will publish again. Ten or fifteen persons out of the hundred are on every list we have encountered. They are such giants that no matter what text we apply, their names will appear. This is what I originally conceived as the "invisible college" - the core group.

In this particular sample the 15 out of 100 is in part an artifact of the sample. Of the 47 persons, ten had published sometime before, but did not happen to appear in the last index. If we merely compared the last index with the present index, we would regard these as new names. The 22 names which appeared last year appear again this year, but they are not going to appear every year in the future. It appears that the number who produce one paper, stay for one year's index, or appear as cited just once are high, but the number who remain for increasing periods of time becomes smaller and smaller. The result of such a distribution is a small, residual group with very high productivity, great impact, and high status.

All these factors are related, and the very large flow through, the enormous mortality and great competition are characteristics which distinguish the groups which we have been studying from other groups. In this presentation, I have tried to provide a matrix for what I think has been going on during the last four or five years that we have studied the invisible college.

Discussion

Fred Strodtbeck: At times, you have shifted from your sample to reference to an invisible core. I wonder, having generated it as you did, whether this representative sample would necessarily characterize fields in different stages of development. I am disposed to think that the aging of a field has something to do with the degree to which it attracts people at a time when they were in attractable stages in their own careers.

Derek Price: An overall sample would be in agreement if, and I believe this is true, each specialty grew in much the same way as the population of specialties grew. If you have exponential growth of the population as well as the subsets, then such a sample as a whole grows exponentially.

When a field of science grows to a point where some identifiable part of it contains ten people, one speaks of it as a field. A field is defined by the existence of a social group rather than by the existence of intellectual criteria. If this is true, then we can suppose, at least for the first approximation, that there is an equivalent gross statistical behavior between the totality of science on one hand and any particular subject on the other. In my work with Donald Beaver, we found that the way scientists working in electrolytic phosphorylation behaved was very similar to the way the totality of science behaved.

If you start with a piece of subject matter as Diana Crane did with agricultural sociology, it may or may not correspond with a fixed group of people. At the beginning, it surely does, but eventually, as one can clearly see in physics and in molecular biology, the original classification breaks down and you do not have a subject like that any more. I think that when you see aging, it is an artifact of the way that you have captured the group - by defining an intellectual subject rather than by defining a group of people. In the history of recent physics, for example, there used to be a subject called cosmic ray physics.

Fred Strodtbeck: I anticipated that there were fields that faded off. In connection with movement in and out of research areas, I studied people who were working on non-conservation of parity. We did not need sophisticated equipment to work in this area. Many people who had been working in other areas got into it, who would otherwise not have, until this work began to spread out into two other main areas. I am quite interested in where persons who support scientific fads come from. They spend a few years in a field and then move off. The polywater group, over the past couple of years, is an instance of this sort.

Derek Price: I think I can give a model for concentration of people in a field. Let us view science as a giant cooperative jig-saw puzzle. There is a finite number of people standing around, and a start has already been made in putting it together. There are local areas of popularity where something is happening, where there are clues, and where there are more people standing around with hands reaching out for

the pieces. There are inactive areas where everyone is stymied because no one know where the remaining pieces fit. But the picture is growing and it is worth watching because the clues are fresh and everyone has a fair chance. This model illustrates congregation at local areas which are growing and to which we assign names of subject fields.

Occasionally, a piece is put in the wrong place and then it will grow out. Sometimes you have to dismantle a whole area because the piece on which it was founded was wrongly placed. That is probably what happened in the polywater incident.

Perspectives on Communication Research from the Viewpoint of a Social Scientist

Fred L. Strodtbeck

With respect to your research in the social organization of science, I would like to introduce two notions. First, that the state of maturation of the scientists, especially their social expectations, might interact and influence their information-seeking behavior. Chart 1 shows a model developed by Jane Loevinger, based upon work of Piaget and Harry Stack Sullivan, for the ego development of an individual from childhood through adulthood. The milestones in this model represent points of reorganization in which there are different levels of concern. Having attained one stage, a scientist's strategies and subsequent probing may proceed to another stage. For example, a former student recently returned to the University of Chicago and presented a biographical re-creation of problems he viewed as important during different stages of life. The confidence with which he could describe that he was no longer interested in publishing papers represented a turning point, a kind of maturation syndrome.

The second notion entails rating of interacting scientists on a competition-cooperation scale. In a two-person game, the person who is going to play cooperatively will likely assume that he will encounter other players who cover a spectrum of attitudes. Those who are most disposed to play competitively will assume that the other players in the game will also be competitive. If we are thinking about communication in science, we are thinking about this rather complicated process. Whether a scientist is at the point of playing the game competitively or cooperatively may make a great difference in the way he goes about searching or exchanging information.

Chart 1. Loevinger's
Stages of Ego Development and Responsibility [1]

Stage	Interpersonal Style	Conscious Preoccupation	Character Development	Responsibility
Impulsive I-2	Exploitive, dependent. People evaluated on reaction to S's demands (good-bad).	Bodily feelings, especially sexual and aggressive.	Impulse-ridden, fear of retaliation. Physical punishment orientation.	Diffusion in attribution of causality. Externalization of blame to source of punishment and rewards (parents).
Opportunistic, Self-Protective	Exploitive, manipulative. Zero-sum game.	Protection of self; cautiousness, suspiciousness, projection. Simple instrumental hedonism.	Expedient, fear of being caught. Rules are recognized only in the negative effects that their transgression produces.	Denial of negative responsibility, externalization of blame (fate, circumstance, others).
Conformist I-3	Reciprocal, superficial. Need for acceptance and approval by authority and group.	Things, appearance, reputation. Stereotyped thinking.	Conformity to external rules. Dichotomization of right and wrong, as based on formulae for behavior and on societal standards.	Self blame is limited to concrete actions, and to specific rules, the referent of responsibility is the authority or the law.
Self-Conscious I-3/4	Reciprocal, affective. Relationships are described in terms of feelings. Emphasis on limited, closed groups.	Awareness of feelings. The Self as related to the group. Transition from actions to traits (quasi-traits).	Morality of reciprocity, and of role-taking.	The materiality of the action is replaced by the awareness of the intention, and of their consequences. The referent of responsibility are the others.
Conscientious I-4	Intensive, responsible. Mutuality, respect, duty.	Differentiated inner feelings. Achievement. Traits.	Internalized rules, guilt.	The referent of responsibility is the Self. Emphasis on self-criticism and self-change.
Autonomous I-5	Add: Respect for autonomy.	Vivid feelings integrates physiological and psychological causation, seeks self-fulfillment in social context.	Copes with conflicting inner needs, shows toleration.	Works in conscious presence of conflicting norms, and ambiguity.
Integrated I-6		Add: Identity	Reconciles, renunciation of the unattainable.	

A scientist, during a certain time in his career, may have a real need to publish, and then at a later point he may choose to publish out of a growth motivation. In our studies of socialization strategies for various socioeconomic strata, for example, we found that lower class often say to their children, "If you don't do this, you will be hungry when you are old or get into trouble." This is a kind of need motivation.

There are also phases of motivation which take place in the history of a scientist's adaptation to his field. In playing with this social organization in your studies and the type of hyper-productivity you found, there are some very interesting dynamics associated with the differential range of institutional contacts. Some of the possible dynamics in a laboratory are such that there are persons who have the same faith that you have, who stand in equal jeopardy of not gaining tenure, or are not being well-received by persons in authority. However, people who are in the same location often work out specializations of their own interest so that they do not overlap. These informal models illustrate the complexity of the situations with which you are working.

If you have a person publishing through need making an inquiry to a person publishing through growth -- that is a pretty congenial way to interact. If a growth person turns to a need person -- that kind of relationship may involve someone who is at a point of competence where he has moved away from the "paper every six months" orientation. He turns to some of the graduate students who are working with him where he can make a kind of exchange of giving protection in academia for something like competence in the new computer techniques. On the other hand, where growth competence is involved, a growth person may not get much help from

a need person. Then there is the growth person interacting with a growth person where the focus is only on ways in which they will progressively differentiate their concerns. It is the autonomy and the uniqueness of their emphases which tend toward congenial communication, even though each in his own work is probably pushing toward a grant. Those relationships which, regardless of growth state, are not strongly affected by personality differences are not necessarily anticipated in this paradigm.

Suppose we organize people in a community in terms of the degree to which they are impulsive, defensive, conforming, conscientious, autonomous, or integrated. We will likely find that it is the conforming conscientious person who is really attempting to live with the rules and who undertake something only where there is a great certainty of producing a tangible, pragmatic result. The persons who are less conforming are more disposed to choose what may seem to be an insoluble problem, but one which may have broader implications if he could find out something about it. It is my feeling that these people are found where greater risks can be entertained.

There are many of us who, if we cannot write our articles in such a way that there is a high probability of their being reprinted because of the degree to which they stop or turn, we could not keep up our motivation to get them through. Essentially, we feel that the shop work of reporting the things we see easily is probably in good hands, and we have a narcissistic conception of our own roles - which may also cause us to get into binds. What I am asking is: what would be some of the implications that would follow from the kinds of concerns a person has.

I have a few summary notes on the data-generating techniques used in your studies. I am disposed to believe that you have gotten your information inexpensively, and that is it a little too thin to illuminate the process I am talking about. By a slight expansion of the data I think that you are going to open areas for investigation that may otherwise be closed. For example, one could find out, before a paper is submitted to a journal, how the authors' friends actually reviewed the paper and commented on it, as well as the nature of the social relationships. I think that there are few authors who are going well enough that they send them out on a wide-spread basis with the expectation that all others would reciprocate; many send them out on a tentative basis. I, therefore, want to focus your attention on the management and evaluation anxiety on the part of the operating sciences. My feeling is that communication intervention could effectively affect the creative process by administering to the evaluation anxiety as fruitfully as possible.

A second very important point is the degree in which focus is maintained among a set of co-workers. If you are located in a place that is called a "health institute" or something like that, the narrower is the focus and the funds which are available. Work is done in a hurry. The kinds and range of focus must also be understood -- every person must feel he is doing his job in a legitimate way, and then beyond that legitimation, he has a certain range for creative work.

Reference

1. Adapted from Loevinger by F.L. Strodtbeck in "Societal Complexity and Ego-Development: A Structural and Psychological Interdependence," prepared for Methodological Problems in Comparative Sociological Research, a Conference of the Institute for Comparative Sociology, April 8-9, 1971, Bloomington, Indiana

Discussion

Derek Price: You have focused on an extremely important area, but I am inclined to think that physics, chemistry and mathematics in contrast with the medical sciences have pathological differences that may not be apparent to social scientists. It seems to me that the objectivity of creative discovery is like the discovery of plants rather than making a painting or bringing to bear new human wisdom and understanding on nature, and an act of discovery of this type is not complete without being broadcast for peer judgement. Unless you put it up for competition and for cooperation, you do not and cannot know what you have created. In general, the psychology works in such a way that you have an area of legitimate discovery and contribution only if you are competing with others. If you have somehow gone out and done something different from everybody else, you are perverse. If you keep it secret and tell only your friends or the school that you are concerned with, then you know that you haven't made a proper discovery because it is not acquiesced to by the entire peerage.

There is a great deal of normative judgement of different sorts made not only about legitimacy, but of the amount of contribution that is being made. The paradox that you win private intellectual property by open publication is rather essential to the function of publication. As a result a great deal of alleged communication is in fact mechanism for publication and evaluation rather than acquisition of information. What is not there is a

need to read papers or to get information. This is at a very secondary level. What is there foremost is an urge to secure your intellectual property by publication. We need scientific journals to publish in, we need an evaluation mechanism to go along with it, but we do not desire particularly for people to read. In this way, science is different from the rest of the community.

In the game theory analogy, science is clearly not a zero sum game situation. When two scientists impinge upon one another in a competitive situation the impact is not like that of two billiard balls, where one gives up energy and the other gains. When the two meet, one may gain very much more than the other loses. It is not simply a matter of how much information one scientist can take from another who has it. When two scientists are competing for the same new law, new piece of information, or attracting a bright new student or getting time on the machine, the one who wins has his power increased so that he is more likely to win the next time. The competition is fierce and people are not only climbing on each others' shoulders, but trampling the one behind in the dust. I think that this has a lot to do with the excessive attrition that was in the invisible college situation.

If one compared the soft sciences, soft in the sense that is is humanistic, personal, and judgement-bound, with high energy physics or molecular biology, then you find great differences.

The entire communication system is such that hard science is designed to maximize output with almost no attention paid at the formal level to input, which is left to the evaluative function. Within a discipline there virtually does not exist a formal input mechanism; there is only a formal output machinery and hardly anyone reads journals to acquire information. If anything like this is true, then the invisible college is the closest we have come to maximizing the flow of information. If you really want to communicate information you must attend to the mechanics of an informal machinery and leave the formal machinery to its quite different function, that of clearing one's desk, establishing private property, or communicating outside the group as from theoreticians to practitioners.

I have a hunch also that science is a sort of conspiracy to make knowledge run much faster than any individual can. If this is true, it explains why the bright man on top of the hierarchy is forever leap-frogging. You get into an area of research, you are on the front line bringing graduate students up to it, and suddenly it has run ahead beyond you. Einstein goes from Brownian motion to relativity or Maxwell goes from electromagnetic theory to physical thermodynamics. This is quite characteristic throughout the history of science. The Crick and Watson jump out of physics into molecular biology is not an anomaly.

The boundary problem of science is such that there is much jumping from highly circumscribed, self-contained invisible colleges to quite new ones. Training is therefore not a substitute for

education; we simply have to educate scientists to leapfrog into a succession of major fields, rather than to train them for particular disciplines.

Diana Crane: I think the phenomenon we are looking at has another dimension. Research fields vary in the number of people they get, and a very young field might have a lot of "need-type" individuals, but I still think they are going to circulate their reprints because they want to get informal priority from circulation. This is one way they do it because they can't get into print fast enough to establish priority. In my own area, if I know that someone is working on the same sort of data, I do not want a person to see my paper before it has been accepted.

Derek Price: One can observe in developing countries where the effectiveness of the invisible college can be impeded by factors such as the size of the scientific community. Where it is too small, you lose your autonomy and your peer judgement facility, as the group is too accidental, too idiosyncratic, and you know each other too well. You can't send a paper to this person because he is competing for the same job you are, or you had a big argument with his sister. In a successful invisible college you have objectivity as well as good communication.

Fred Strodtbeck: I confirm your analysis of the small communities which are manifestly competitive when you have a large enough co-activity operating. When you speak of the 43 people who are pushed out at certain phases, there must be some potential contribution in those 43 which may otherwise be tapped.

Derek Price: It is roughly like this. Let us say there are 100 people in a given field, then seven new ones are grown each year. This entails having an order of magnitude of 30 graduate students in the incubator and 30 people teaching. It means you have got 70 left over. Fields differ enormously in just this sort of division, as when physics is compared with history.

In history, the seven are all turned back into graduate school or college teaching. Society is paying for people to go through a history education to become teachers to educate a great mass of students who will in turn educate more students. In the sciences a quite different social mechanism is involved. In chemical education, for example, 25-30 percent feedback is sufficient to maintain the teaching apparatus and there is an output of around two out of three who go into other activity than producing chemical scholarships. The universities have not yet paid much attention to what is the main function of historians. These factors contribute to differences at the research front. In the sciences, most of the jobs are not in academia and a certain proportion of the people go away from this highly competitive situation. "Wastage" from the research front means that someone has found another professional career.

Diana Crane: You might also argue that perhaps people who do not like competition, the "non-need" types, would leave the field.

Derek Price: From the little we know about personality, the ones who

remain in scientific research tend to be the ones who want the competition without the interpersonal relations. Whereas, if they are effective with people and like personal interaction they become not physicists but engineers, doctors, lawyers or some occupation with a social function. They don't produce the objective scholarship.

Another characteristic of physics, chemistry or biology is the lack of choice -- there is a certain sense of no two ways of doing business. Either you are doing it the universal way, obeying universal norms in problems or you are not doing it at all. In a very strong sense you can't even have very much of a different strata, The difference amounts only to different personalities treating the big apparatus on the East coast and the West coast of the United States.

Seen from the viewpoint of an individual physicist, one can choose which part of the jig saw puzzle to play, but once you are there (which is in part an integral of where you have been), there is really only one way to go. In subjects that are a little "softer", you can have schools of thought, schools of interest and schools of philosophy. The concept is practically nonexistent in physics and chemistry. Chemists, whether they are in research organizations or pharmaceutical companies, can work on different parts of chemistry, such as steroids or testosterones, but once you choose there is not much to it. There is only one way to play it

and a Lithuanian pharmaceutical chemist who is working on steroids has the same equipment, the same knowledge and the same journals. If he is doing anything different he is not where you are.

There is a marvelous piece where Galileo cries bitterly about how awful it is that there are no longer classics - you have to read books by people who are actually alive. We have been increasing the pace ever since and it is this that gave birth to the scientific paper in the mid-17th century and the abstract and bibliography mechanism in the 18th century. It is now giving rise to the informal communication mechanism of the 20th century.

Summaries of

Current or Completed Research

Invisible Colleges and Social Circles: the Sociology of Scientific Growth

Diana Crane

The book which I have recently completed is an attempt to review the literature on the social organization of research areas in order to assess what is known about the social factors affecting the growth of scientific knowledge.

The model which is presented in the book is one which interprets science as consisting of hundreds of research areas at various stages of growth. The development of those areas which attract many members follows the stages of the logistic growth curve. At any point in time, there are research areas at each stage. Some are very small and may never produce findings of sufficient interest to stimulate a period of rapid expansion. Others are undergoing rapid growth while still others are declining. Some areas have passed through all four stages of growth and have been abandoned by all but a few diehards. In time, it is possible that new findings revivify these fields.

The relationships between scientists within such areas have a definite structure which changes over time. In the first stage, an area has few members who have little contact with one another. Some of them produce theoretical and experimental research of exceptional interest. These ideas are sufficiently compelling to attract new members to the area and to provide them with a definition of future research priorities in the area. These orientations vary considerably

in the degree of confidence which scientists can place in them. They may range from expectations regarding the nature of phenomena to highly sophisticated paradigms.

The emergence of a "paradigm" and of the beginnings of social organization in an area triggers a period of exponential growth. This growth results from a contagion process in which information is relayed to steadily increasing numbers of individuals, until all those who have any potential interest in the field have been contacted. To scientists working in other areas who have not been impressed by the "paradigm", this period of exponential growth may be perceived as a "fashion", a phenomenon created solely by mutual influence and lacking any theoretical or empirical validity.

Among those who are attracted to the area are a few scientists who develop a long-term commitment to it and who become very productive. They train students in the area and collaborate with them and with other scientists. Their activities are an important factor in contributing to the period of exponential growth. Their work provides a reference point for that of others. Since the scientist must rely on his peers for recognition (which they provide in part by citing his work in their publications), he is motivated to conform to the standards which the group develops concerning the selection of research problems.

The diffusion of ideas within the area is accelerated by the activities of the "opinion leaders" and by the communication network which develops within the group. The acceptance of widely adopted innovations also follows the logistic growth curve. Subsequent

research builds upon the earlier innovations and remains closely related to them. Much of the later work does not generate new research.

The more novel or ambiguous the paradigm, the more likely the groups of collaborators which form in the area will behave in some ways like social movements, providing ideologies to support their cause and social support to their members. In these instances, norms regarding the evaluation of knowledge will be less rigorously enforced: hypotheses may be accepted on faith; preference may be given to the scientist's students and collaborators; emotional involvement with an idea may occur; information may be kept secret until priority can be established.

Time has a similar effect. The leading figures may develop increasingly differentiated orientations toward the field. As a result, groups of collaborators are less likely to accept each others' ideas. In some cases, these groups become "schools", heavily committed to their own viewpoints and in conflict with one another. Young scientists looking for research areas are less likely to select an area where this phenomenon has occurred, since they can see that an establishment already exists which can be expected to resist new ideas. It is also possible that other areas cease to accept the ideas produced by such a field.

Alternatively, a new paradigm emerges and attracts scientists to the area, thus producing another period of exponential growth. In other cases, the implications of a paradigm are simply exhausted. Scientists in research areas are usually committed more to the solution of the problem than to the group itself. The research area

can best be understood as a temporary unit which deals with special problems and then dissolves after a period of time when the problems have either been solved or been determined to be unsolvable. Bennis (1966) has argued that this type of organization is the prototype of an organization designed to produce innovation.

Although they have tendencies toward closure and orthodoxy, research areas are generally open to influence from other areas. Members have a wide range of connections with members of closely related and even quite distant areas. The structure of these relationships is as yet poorly understood. It has considerable implications for our understanding of the development of new areas and the diffusion of information from one area to another.

This model is an attempt to draw together findings from a number of studies in different research specialties (sociology of science, history of science, information science) and to show that these findings can be interpreted in terms of a single framework. A number of aspects of this model remain to be examined. For example, information is needed about the growth processes of a much larger number of research areas in various disciplines. Are there variations in the growth process? What factors stimulate the growth of research areas? What types of scientists are most likely to enter a new area and develop it? What motivates them to do so?

Mulkay (1969) has suggested that new fields develop because scientists discover new phenomena to which an old paradigm can be applied. Kuhn has stressed the revolutionary aspects of scientific change. Alternatively, change may occur when the relevance of

different paradigms to one another is grasped. Such radical combinations may stimulate rapid growth. There is some indication that this occurred in the field of molecular biology, progress in which was made possible by discoveries in several disciplines (Garfield et al., 1964; Hess, 1970). We need to understand much more about how scientists use ideas and how ideas develop and cumulate.

The organizational factors which make possible the incessant proliferation of scientific knowledge have been largely neglected in this analysis but it is certainly true that under different circumstances the networks described here could not have flourished. Ben-David's studies of national scientific communities (1960,1964, 1965, 1968) indicate that lack of organizational support can inhibit the natural growth of science. Studies of the diffusion of scientific ideas across national boundaries (for example, Zaltman and Köhler, 1970) and of the existence and reasons for the development of national scientific traditions are also necessary. Finally, the implications of this model for practical problems of information science and technology need to be understood.

(To be published by University of Chicago Press)

Informal Communication Among Scientists in Sleep Research

J.Am.Soc. for Information Science 22:301-310, Sept.-Oct.1971

Susan Crawford

At the frontiers of an active area of science, social structure based upon communication is demonstrated. Using sociometric techniques, an informal communication network was identified which included 73% of the scientists. Within the network was a core group of scientists who were the focus of a disproportionately large number of contacts and who were differentiated from others by greater productivity, higher citation record and wider readership. Information transferred to these scientists is so situated that it could be transmitted to 95% of the network scientists through one intermediary scientist or less.

Related Work:

Communication Centrality and Performance.
Proc. Am. Soc. for Information Science, 1970. pp.45-48

The Motivation System of Science

Bernard H. Gustin

The exchange model of science cannot hold, since a large proportion of the scientific community publishes very little, if at all; the elite, prolific scientists are neither dependent on nor rewarded for the publication of their research; and most work by average scientists is never read, much less "rewarded" with citations and prizes. Charisma is proposed as the basis for an alternative model of scientific motivation; its function in the differentiation of the scientific community, linked to the relationship between the published literature and the invisible colleges as mechanisms of scientific communication and to the cumulative pattern of growth of scientific knowledge is outlined. (Mimeo).

A Reconceptualization of the Problem of Scientific Communication

Nicholas Mullins

Sufficient empirical study of scientific communication has been done to provide a description of the major elements involved in that communication. At the level of the individual scientist, we have conceptualized the system as a network which has certain static properties. Attempts to deal with change in communications networks have not been notably successful.

I am presently experimenting with cellular automata theory as a technique to describe changes in the communication system. This theory requires an indefinite, n-dimensional Euclidean space with a neighborhood relation defined on that space. The neighborhood relation gives each element a finite list of neighboring elements. This relation operates on a synchronous time basis. Each cell may be in one of a finite list of states. A rule defines the states of a cell at t+1 for each set of possible states of that cell and its neighbors at t. The list of states for a cell and the rule governing change from that state are a transition function.

Known results in the area are that some patterns can be precisely self-reproducing, expanding, or contracting depending on the transition function chosen. If we conceive the distribution of scientific communication at any point in time as the states of a cellular automata, we may be able to discover rules which govern the

the transition from states of combining information, transmitting it, creating new information, or ceasing to communicate.

References

1. Burks, Arthur W. Essays on Cellular Automata. Urbana, University of Illinois Press, 1970.

2. Arbib, Michael A. Theories of Abstract Automata. Englewood Cliffs, N.J., Prentice-Hall, 1969.

Other Recent Papers

1. The Structure of an Elite: Public Advisory Structure of the Public Health Service, 1957-67. Part I: Macro-structure; Part II: Micro-structure. December 21, 1970 (Mimeo).

2. A Model for Development of a Scientific Specialty: The Page Group and the Origins of Molecular Biology. February 1971 (Mimeo).

Interpersonal Communication Among Educational Researchers

David A. Lingwood

The study is based on data on research contacts, use of scientific media, output of information, and personal and job characteristics of 209 educational researchers (with a sociometric matrix containing 274 persons) (Lingwood, 1969). In the correlational analysis portion of the study, we were interested in examining the interrelations of sociometric integration, information inputs and outputs, and the other characteristics available. Let us select two of the most interesting multiple regression analyses produced.

First, we developed scores for each person, based on the choices each respondent made regarding persons with whom he was in contact about his primary research specialty. Scores were also computed for the choices each respondent received from others. The actual computations performed were Lin's (1968) measure of "sociometric prestige" -- which is based on both direct and indirect linkages in the sociometric matrix. Since scores for choices made are highly subject to biases of nonresponse, let's look at the multiple regression prediction of the choices received measure, using 18 personal, job, and information input predictors.

Fourteen and one half percent of the variance in the sociometric prestige index was accounted for in this analysis (Table 1). Only two of the variables provide significant independent prediction: respondents who use more informal print input channels (papers, pre and reprints, unpublished reports), or those who (unexplicably) rate the same media as of low importance are higher in the choices they receive as measured by the index.

When we move on to prediction of an index of information production (across paper, articles, book and book chapters), again using personal, job, information input, and sociometric indices as predictors (Table 2) we find 23.6 per cent of the variance accounted for. Looking at the variables providing strong independent predictive power, we find those who are high in an index for use of all scientific media, those who work in universities, and those who are high in the index of sociometric prestige based on choices received from others produce the most information. Thus, even with inputs and personal and job characteristics present in the equation, the sociometric index emerges as a strong predictor when the effects of all others are partialled out (a partial R with the d.v. of .23). In a subsidiary analysis, we found the predictive power of the sociometric index was greatest for article and book chapter production, and that sociometric prestige received contributed little to predictions of unpublished paper and book production.

Thus, even though we obtained no clear prediction of who is the most "professionally-visible" researcher, based on choices received, we did demonstrate the importance of contacts among researchers as predictors of information production. Such correlational analysis cannot, of course, help us decide if it is sociometric prestige which leads to higher outputs, or whether the man who is productive comes to have more contacts (or at least, have more people mention him), or a bit of both.

The next portion of my work explored the relation between sociometric choices within the respondent's primary research specialty and the identification he provided for this specialty. Since the nature of an hypothesized invisible college is, in part, scientists interacting interpersonally about a common research topic, there should be a strong overlap between specialties named, and the other researchers contacted; the analysis to determine the extent of overlap was conducted in two ways. First, we coded research specialties mentioned into 13 specialty groups, then compared the amount of actual mention made by respondents in any one group to others also coded into that group. The second analysis simply reversed the process: cluster analysis was used to group respondents on the basis of frequency of personal contact, then the specialty codes given by each member in each cluster were compared in a search for communality of specialty codes within the cluster.

In the first approach, the respondents were grouped into the 13 specialty code groups, then the destination of each group member's

sociometric choices was computed (either going to others sharing that specialty code, or to persons not in the code group.) Using a notion of the number of choices we should expect members of a group to make to others in the same group on the basis of chance alone, a "sociometric Chi-square" was computed for each group.* Eleven out of 13 of the specialty code groups showed greater intra-group choice than chance would predict (See Table 3). The highest Chi-square was for the specialty group we titled "studies of communicative behavior: speaking, reading, and writing skills; psycholinguistics and communication." The 43 persons in the group chose each other 67 times, and others 233 times, while the expected number of intra-group choices predicted totaled only nine. Other very strong self-choosing groups were: "theory of technology," and "guidance and counseling."

In the second analysis, several methods of producing sociometric cliques were attempted, without success in separating the rather

---

* The expected value is derived as follows: assuming any person i in a choosing group makes nc_i choices, and we wish to know what proportion of these choices should be given to a randomly constructed sub-group of size m, and that there are n persons in the choosing group, the expected number of choices we should find going from the chooser group to the chosen group is expressed as

$$E = \sum_{i=1}^{n} \frac{nc_i \cdot m}{N-1}$$

(where N is the total group size).

If we are interested in knowing how many choices members of a sub-group should make back into their own sub-group, the values n and m are both equivalenced to the size of the sub-group in question. In the present study N may be considered to be either the size of the sociometric matrix, or the total number of persons mentioned (both inside and outside of the matrix). Comparisons of expected values computed for both of these sizes with obtained inside-group choices for many randomly constructed groups showed that the latter estimate for N (all persons mentioned) provided the more accurate estimate.

cohesive total matrix. Finally, a two-stage cluster analysis was used, clustering respondents together on the basis of frequency of personal contact about the primary research specialty. The first round of clustering produced 65 very small clusters (some only two persons), but when a matrix of inter-cluster association was itself cluster analyzed, a final set of 19 groups was produced, ranging in size from 16 persons to only five. Lacking a model of how many common mentions of research specialty codes we should expect in any cluster, only rough comparisons can be made. The first clustering produced little tendency for cluster members to mention the same specialty codes, however (Table 4). One problem here might be the effect of physical proximity on frequency of contact. So, frequency of contact scores were regressed for physical distance between choosing pairs (there was a -.14 $\underline{r}$ between frequency of contact and distance). With the distance effects removed, and the two-stage clustering repeated, the clustered persons showed a greater tendency to mention the same specialty code. Six of the 18 clusters produced showed a preponderance (i.e., more than half) of members sharing one specialty code (Table 5). For example, the first cluster was composed of nine persons, seven of whom mentioned the code "teaching technology", while the remaining two either provided no data or were uncodable.

The two analyses thus tend to agree that there is some correspondence between sociometric choice patterns and patterns of names given research specialties. The groups were generally small in both analyses, and we cannot conclude that any invisible colleges were

completely defined. I will argue below, however, that isolation of more or less self-contained invisible colleges is primarily important only to verification of the hypothesis, and that the methodology of our common studies may be useful for applied purposes, assuming that some general housekeeping is first done with the methodology itself.

Methodological problems

Sociometric analysis and the invisible college hypothesis are closely linked; the necessity to find groups of interacting scientists often requires the researcher to perform the most difficult variety of sociometric analysis, clique detection. Clique detection usually becomes the researcher's mode of perception, in which he attempts to separate the invisible college "figure" from the "background" of his overall sample. I will outline several problems here, however; related to sociometric methods themselves, and to the effect of data collection on sociometric methods. Let me just list several of these problems.

First, the "snowball" method of data gathering, or of sample-expanding is often used to find additions who a are mentioned by b who are mentioned as sharing a specialty. It would seem that sociometric analysis of sample members added because of their degree of contact with previous members would tend to overestimate the degree of cohesiveness of the group. This danger is one reason why I examined the convergence of sociometric choice and specialty names in the research summarized earlier, but even this approach is no necessary guarantee that the sampling has not biased the data

toward almost automatic inclusion of small, highly cohesive groups. It would be better if some external information were available for sample construction, such as association interest group membership, etc. -- criteria which will, hopefully, be wider than the boundaries of one invisible college.

The next point (or rather, series of related points) has to do with the methods used to form cliques. First, I think all of our experience has shown that the older, basically pre-computer cliquing methods are simply inadequate to the task of reducing large socio-matrices to small cliques. In particular, sociograms and other matrix manipulation heuristic approaches (e.g., those forming groups either along or away from the major diagonal) imply that the order n sociogram can be represented in a two dimensional space. In addition, little research has been done on the utility of those methods for reducing the order n matrix to some m dimensional space (other than some work in binary factor analysis). Beyond factor and cluster analysis, smallest space analysis or multidimensional scaling might be attempted. The key is not just use, however, but comparison of several methods on the same data sets (possibly contrived ones with known structure). Beyond this, little work has been done on the effects on the clique production of data characteristics such as binary vs scalar cell values, symmetric vs asymmetric assumptions and models, free vs restricted sociometric choice formats in questionnaires, and the effect of missing data.

Without such methodological improvement in sociometric clique production, it is tempting to suggest that we should refrain from this approach, and concentrate on the class of sociometric procedures which

simply gives individuals scores for their interaction with others, then relate these scores to whatever variables are necessary to infer invisible college membership.

Transaction Flows and Diffusion of Research Specialties in an International Scientific Community

Gerald Zaltman and Barbara M. Köhler

This paper investigates the concept of internationality in the social structure of science. Theoretical high energy physics, a so-called "hot" area in physics, is the particular field being studied. Data collected from 977 high energy physicists working in 38 countries are presented. The paper examines first the allocation of manpower in this field by country of employment and nationality and by institution. Next the distribution of professional recognition as measured by research leadership (a measure of formal recognition) and advisorship (a measure of informal recognition) are studied, the journal awareness of the physicists is examined and, finally, the diffusion of research specialties among the major geopolitical units is discussed. The analysis suggests that there are distinguishable social systems in theoretical high energy physics but that the ties among these systems are sufficiently strong so as to form a larger international social system. Formal and informal recognition flow relatively unimpeded by national or cultural barriers and with some exceptions the diffusion of research specialties also takes place without regard to economic, political or cultural boundaries. The data also suggest that leadership in theoretical high energy physics may be shifting from the United States to Japan. (Mimeo).

Differential Productivity of Colleague Groups at Two Research Fronts

P. David Vachon

This research began with the observation by Drs. Derek de Solla Price and Donald deB. Beaver that three indices of the productivity of colleague groups of scientists in the National Institutes of Health - sponsored "Information Exchange Group No. 1" (IEG-1) varied with the size of the colleague group. The indices were "papers per man", "authorships per man" and "authorships per paper." The question was whether analysis of the social structure and functioning of science when applied to the collaboration of these biochemists in colleague groups could account for the apparent functional dependency of the Price-Beaver productivity indices on group size.

The explanatory sociological variables introduced were "closeness to the research frontier" "departmental status" and "proportion of theoretical output." A second, vastly different population was selected as a stringent comparative test of the generality of any conclusions reached from the study of IEG-1. This second group consisted of the worldwide population of nuclear physicists who worked on the proposal advanced by T. D. Lee and C. N. Yang in 1956 that parity was not conserved.

A theoretical model relating the sociological variables introduced to the effect observed by Price and Beaver was deduced by applying a general theoretical "social reality paradigm" developed elsewhere by by the author to middle-range sociology of science results. The model

was expressed both verbally and as a path analysis diagram. The techniques of Colon Classification were applied to the deductive chain of reasoning to determine how much of the final model was supported by these sociology of science results. Since closeness to the research frontier was a concept that had been implied in the literature, but since no results had been published about it, a hypothesis had to be introduced to connect it to the other variables studied. That hypothesis was that closeness to the research frontier acts as an intervening variable between group size and productivity.

The empirical portion of this study was conducted entirely by unobtrusive techniques involving literature research and citation analysis. Indices of closeness to the research frontier were developed from Dr. James S. Coleman's "Sociometric Connectedness" program as refined by Eric J. Steiner. A parallel index based on survey research data compiled by Dr. Errett Albritton also was developed for the IEG-1 population. Indices of departmental status were based on the work of Bernard Berelson, Hayward Keniston and Allan M. Cartter. For the physics group the proportion of theoretical output index was based on an examination of the nature of the papers produced by each group.

Our central result was that our hypothesis was not maintained. Rather, a dependence chain starting with departmental status continuing through closeness to the research frontier and group size and ending in group productivity measured in papers proved correct.

It was also found that the indices advanced by Price and Beaver were statistically unacceptable for the multiple linear regression

technique used in the path analysis test of our model. The "proportion of theoretical output" index also proved invalid. That the strongest dependencies for closeness to the research frontier were found for indices drawn from records of informal communication showed the dynamic nature of the research frontiers being explored by these two populations of scientists.

The relation of this research to current questions in the sociology of science also was analyzed. These questions involved measurement of the quality of individual scientists' research, identification of invisible colleges and the validity of Kuhn's paradigmatic model of the growth of a science.